

which he was associated would have occurred in the valley-gravels of Scotland, Ireland, and the northern and midland counties of England, just as in those of the south-east." Mr. Geikie makes a similar statement in his preface:—"A wide land-surface existed in the British area after the disappearance of the ice-sheet and before the period of great submergence;" and he cites the discovery of the human fibula under glacial clay in the Victoria Cave in confirmation.

It has always seemed to me that in discarding the power of the ice-sheet for that of the "great submergence" as an agent for the removal of all traces of an earlier fauna, Mr. Geikie, when attacking the tree of prejudice, has cast down his axe and taken up a whittle. Apart from the very doubtful extent and depth of the submergence, its destructive powers cannot for completeness be compared to that of the grinding of an ice-sheet. In a submergence, even if the beating of the surf destroyed all superficial deposits—a supposition which, if applied, a coast so abounding in land-locked and sheltered firths as Scotland partly submerged would present, is in the highest degree improbable—the rivers at least would carry down carcasses into secure resting-places and entomb them in estuary mud, and it would be most unlikely that no such relics should be preserved when the land rose again. But, on the other hand, it is difficult to believe that any organic remains could escape the grinding of an ice-sheet if continued through a long period.

In the Victoria Cave, at any rate, the surroundings are such that nothing but an ice-sheet could have sealed up with glacial clay the remains discovered by the Committee. The valley lies close by, but is 900 feet deeper, and no advance of a mere valley glacier in the supposed later increase of glacial conditions could have brought the boulders to that height. The form and situation of the hill near the top of which the cave lies is such, that no small ice-field could have formed on it and brought this glacial debris. The origin of the boulders, their position, the ice-scratches on the rocks hard by, all point to the time of greatest glaciation when the whole district probably was covered in with ice and snow of great thickness. And the agent which closed the cavern and concealed the animals within it must have been the same which swept the country clean of their remains all around further than the eye can reach.

To sum up, the direct evidences as yet found to support, by actual infraposition, the inter-glacial age of palæolithic man and of the fauna with which he is associated, are as follows:—

1. Victoria Cave, Settle:—A human fibula under glacial till, and associated with *Elephas antiquus*, *Rhinoceros leptorhinus*, *Hyæna*, *Hippopotamus*, &c.¹

2. At Wetzikon, Canton Zurich, a piece of lignite containing basket-work lying beneath glacial deposits, and associated with *Elephas antiquus* and *Rhinoceros leptorhinus*.²

3. Near Brandon, Suffolk, implements, with bones not yet determined, in brick-earth beneath the great chalky boulder-clay of East Anglia.

There is nothing in any of these instances to support the notion that this particular fauna lived subsequently to the age of the Scottish ice-sheet and immediately prior to a great submergence.

The Settle till is undoubtedly of the age of the ice-sheet. The Wetzikon lignite lies upon a glacial till beneath a river gravel, and upon that are huge angular erratic blocks, "clearly indicating the presence of a great glacier posterior in date to the organic remains."³

The Brandon implements are beneath the chalky boulder clay which Mr. Seales Wood, jun., believes to be the product of an ice-sheet, though partly deposited beneath the sea, a condition which is incompatible with the co-existence of a great submergence.

After, and in sole opposition to, such evidence, we can hardly contentedly take the existence of frog and water-rat as upholding the presence of palæolithic man and his congeners in times later than the great ice-sheet of Scotland. The Arctic mammals are, of course, out of court and cannot be taken as evidence, for it is highly probable that they returned with the retreat of the ice; but, so far, we have no evidence that this was the case with the more tropical animals.

My friend Mr. James Geikie will, I am sure, take these sug-

¹ "The Relation of Man to the Ice-sheet in the North of England," NATURE, vol. ix., p. 14, 1873; also "Settle Caves Exploration," Brit. Assoc. Reports for 1874 and 1875.

² Rüttemeyer; Archiv für Anthropologie, 1875; also NATURE, vol. xiii. p. 130.

³ Lyell; "Antiquity of Man," p. 368.

gestions in the friendly spirit in which they are offered. My chief reason for bringing them forward is that we hear that a new edition of his valuable work is in preparation, and it will be a loss to geology if this matter be not fully discussed by one who is so well able to handle the subject in all its bearings. Meanwhile, we are deeply indebted to him for progress already made, and also to my friend Mr. Skertchley for this important addition to the evidence and the perspicuous manner in which he has brought it before us.

R. H. TIDDEMAN

The Flame of Chloride of Sodium in a Common Coal Fire

SOME time ago a correspondent of NATURE (vol. xiii. p. 287) inquired for an explanation of the fact that while common salt (chloride of sodium) colours the flame of an ordinary spirit-lamp yellow, the same substance thrown upon a common coal fire gives rise to a blue flame. In the next number (p. 306) Dr. Schuster stated that the origin of the blue flame was still involved in mystery, and (if my memory is correct, for I have not the number at hand) that he and Prof. Schorlemmer had been engaged in an investigation of the same.

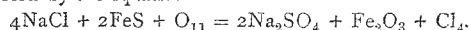
Dr. Schuster's letter shows that the question is not an unimportant one; and as I have lately made a few experiments which seem to confirm an explanation which occurred to me at the time, I send a short description of them.

The theory I put forward is that the blue flame noticed when salt is thrown upon a coal fire (of bituminous coal) is possibly due to the presence of carbonic oxide (CO), produced by a series of reactions, through which the common salt is converted into, first, sulphate, and then sulphide of sodium, as in the manufacture of crude carbonate of sodium (*black ash*), all the reactions being simply carried out in one furnace instead of two.

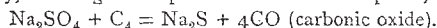
Leblanc's process consists in—1. Converting common salt into sulphate of sodium. 2. The "salt cake" is then mixed with coal and limestone, placed in a furnace and heated strongly, during which process a blue flame of carbonic oxide is observed to play upon the surface.

Now in the case we have under consideration, the only difference is that the salt is first converted into sulphate by the oxidation of the iron pyrites, from which no coal is free (and, in fact, it has been proposed to use such a process commercially, viz., by roasting common salt with iron pyrites).

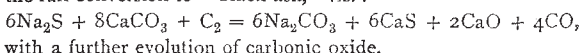
At this stage, then, the reaction going on in the fire will be expressed by the equation—



Almost simultaneously with this, the carbon of the coal comes into play, reducing the sulphate of sodium to sulphide, thus—



Of course were any substance present answering to the limestone used in practice, as may occur in the ash, we should have the full conversion to "black ash," viz.:—



with a further evolution of carbonic oxide.

I need not say that carbonic oxide burns with a violet blue flame perfectly indistinguishable from that produced by throwing salt into a bituminous coal fire. This may be proved at once by experimenting with a fire of anthracite, which itself only gives the slight lambent blue flame of carbonic oxide. The presence of salt makes no difference whatever in the colour of this flame, and it is difficult indeed to determine whether the salt is ignited at all. The difference in the two cases is just this:—A common coal fire has usually a large, bright, or smoky flame. Salt thrown on it diminishes its size and brightness by robbing it of free carbon or hydrocarbon—which gave it those qualities, and which is derived from the volatile matter—as in the reactions above set forth, the result being the production of carbonic oxide. In the case of anthracite, however, the free carbon is absent; but the carbonic oxide exists, and is equally apparent before and after the addition of salt. It is possible that the heat, instead of volatilising the sodium compounds and so giving the yellow flame, is expended in effecting the new chemical transformations.

In the case of a spirit-lamp or of a Bunsen burner there is no supply of carbon, nor is there any iron pyrites to be converted into sulphuric acid, consequently the above complicated process cannot take place, and the flame only shows the sodium coloration.

The following laboratory experiments were made with a view to test the accuracy of these speculations:—

1. A little common salt was placed in a crucible, inclosed in a jacket, and exposed to the Bunsen flame. The fringe of flame appearing above the crucible was of course coloured most intensely yellow.

2. A similar arrangement was made, only that the crucible contained a mixture of common salt and powdered charcoal. Although the crucible was heated to a redness, the flame had lost most notably its intense yellow colour and occasionally a slight blue tinge appeared around the edge. This last I do not lay much stress on, as it might be merely due to the Bunsen flame; but the diminution of the sodium colour could not be overlooked.

3. The crucible was now filled with a mixture of salt and powdered charcoal, together with a very little of sulphide of iron (in fact, the substance used for the preparation of sulphuretted hydrogen), and exposed over the Bunsen burner as before. In this case the sodium coloration almost completely disappeared, while the blue flame became very distinct indeed.

No difference could be observed, whether the air was turned on or off the burner, in these experiments.

When the above mixtures were exposed on platinum wire in the naked flame, they only gave the sodium colour. This is doubtless to be ascribed to the stronger heat volatilising some of the sodium salt before it had time to enter into the necessary changes. This is the more likely, because mixtures made with just the slightest trace of salt gave the yellow colour in the naked flame, while the mixtures used in the crucible as described, and which gave the blue colour, contained fully 50 per cent. of salt.

Dr. Schuster, in the note already mentioned, refers to a paper by Dr. Gladstone (*Phil. Mag.*, 1862, vol. xxiv., p. 417), on the similar behaviour of certain metallic chlorides in imparting a blue or violet colour to flames of various kinds. I find that in this paper the violet colour given by the chlorides of potassium, sodium, and barium, in the flame of red-hot coals is noticed, Dr. Gladstone remarks, however, that "a doubt must rest on such observations made with a common coal fire, as it is quite conceivable that these chlorides may give up their chlorine to the alkalis or earths of the ash."

It struck me that it would have some bearing on the matter, to ascertain if other salts of sodium exhibit the same phenomenon. On trial I find that there is no difference.

A little pure sulphate or carbonate of sodium thrown on a coal fire produces exactly the same blue flame as common salt, both with ordinary coal and with anthracite. These salts, in the flame of the Bunsen or the spirit-lamp, give the strong yellow flame of sodium at once. It is clear that their behaviour on hot coals is explainable in exactly the same way as that of common salt, viz., by the production of carbonic oxide. It is inferable, therefore, that the blue flame of common salt is not to be ascribed solely to some property inherent in chlorides alone; and the solution I have proposed seems the more plausible.

A correspondent in NATURE suggested the probable formation by a reaction between common salt and *copper pyrites* in the coal, of chloride of copper, and that the last would give the blue flame. However, it is iron pyrites, and not copper pyrites, that occurs in coal; and, moreover, the flame of copper chloride is bluish green, and not blue.

Kilkenny

Her Majesty's Geological Survey

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET QUESTION.—Notwithstanding the suspicious aspect of the spot remarked upon the sun's disk by Weber at Peckeloh, on the afternoon of April 4, 1876, as it is described by him in his letters to Profs. Heis and Wolf, it would appear that it must be relegated to that class of ordinary solar spots which are better defined than in the majority of cases, and continue visible but a short time. A letter has been addressed to the Abbé Moigno by Señor Ventosa, of the Observatory at Madrid, containing a very definite observation of a spot on that day which was evidently the one noticed at Peckeloh. A similar letter to Prof. Peters is published in *Ast. Nach.*, No. 2106.

It is not, perhaps, generally known in the astronomical world that the systematic observation of the sun's disk forms one of the routine subjects to which attention is directed at the Madrid Observatory. The observations are made daily with the large Merz-Equatorial, projecting

the image of the sun upon a screen so as to present one of considerable diameter. The heliographic positions of the spots are determined on the method adopted by the late Mr. Carrington on the image projected by the finder, which is provided with a suitable reticule, and, whenever possible, their distances from the limb are measured directly with the large telescope. The drawings are made by hand.

After noon on April 3 the sun was without spot, a group of *facula* only being visible very near the S.W. limb. But on the morning of April 4 there was a small spot, a simple nucleus without penumbra, of an apparently elliptical figure, with a small *facula* on the N.W. side (puro nucleo sin penumbra, de figura elíptica aparentemente, y con una *fácula* pequeña por el lado N.O.); this was very well observed. Cirrus was scattered over parts of the sky, but the images were well defined. The observations gave the following results:—

April 3 at 22h. 9m. 54s. M.T., at Madrid, angle of position of the spot, $76^{\circ} 43'$, distance from the centre of the disc $818''\cdot 9$, the sun's semi-diameter being taken at $960''\cdot 9$. At 22h. 24m. direct measure of the distance of the spot from the sun's limb gave $147''$, consequently $814''$ for the distance from the centre. The dimensions of the spot were $4'' \times 2''$.

April 5, after noon, the sun was again without spot; the most remarkable object was a bright *facula* very near the N.E. limb. It will be seen that the first Madrid observation was made 5h. 7m. previous to that at Peckeloh.

The opinion expressed by M. Leverrier before receiving the Madrid observation, that certain problematical solar-spot observations upon record might accord with a revolution of an intra-mercurial planet in about twenty-eight days, in which case an inferior conjunction might fall on the 2nd or 3rd of the present month, has been construed into a definite prediction of a transit of the so-called *Vulcan*, on one of those days, a prediction which M. Leverrier distinctly repudiates, though it has been widely circulated by the daily press in and out of France. The rejection of the observation of April 4 in the present year leaves us in doubt again as to what period will correspond to the most reliable data, assuming the existence of an inter-mercurial planet.

Mr. Wray's observations about midsummer, 1847, and others of hardly less authority, require explanation. It is impossible to repudiate them, but whether referable to the passage of planetary or cometary bodies, must remain for future decision.

THE VARIABLE STAR, ALGOL.—The following are Greenwich times of visible geocentric minima of Algol to the end of the year, calculated from the elements in Prof. Schönfeld's latest catalogue of variable stars:—

	h. m.		h. m.
Oct. 20	16 42	Dec. 2	16 55
23	13 21	5	13 44
26	10 20	8	10 33
29	7 8	11	7 22
Nov. 12	15 13	22	18 38
15	12 2	25	15 26
18	8 51	28	12 15
21	5 40		

THE MINOR PLANETS.—It appears by a telegram from Vienna in the *Paris Bulletin International*, of September 23, that Herr Palisa, of the Observatory at Pola, has recovered No. 66 of the group of small planets—Maia—detected by Mr. Tuttle, at Cambridge, U.S., though at some distance from the position given in the *Berliner Jahrbuch* for 1878.

The following names are proposed for recent discoveries:—No. 165, discovered August 9, 1876, Loreley; No. 166, August 15, Rhodope; No. 167, August 28, Urda. Nos. 168 and 169 are announced; the former was detected by Prof. Watson on September 27, and the latter by M. Prosper Henry on the following night near the same position. Both are eleventh magnitudes.